VOLUME XLIV, No. 12 JUNE 5, 1947

THE JOURNAL OF PHILOSOPHY

COUNTING, A PHILOSOPHICAL ANALYSIS

INTRODUCTION. How are the principles of arithmetic used in the empirical propositions of everyday life and of the physical sciences? A full answer to this question would require a detailed study of counting and of measurement. In this paper I shall confine myself to counting. I believe that a study of counting illustrates in a useful way the part played by a set of fairly precise terms with interlocking definitions, in making statements that are not analytical; and the way in which it is determined that the definitive rules have (or have not) been used consistently. The study also suggests incidentally the way in which the application of arithmetical principles contributes to the important notion of a continuing physical object.

I shall begin my study of counting by writing out certain propositions which (I believe) everyone would accept as necessary and analytic. These are not offered as a basis for a system of necessary truths about the results of counting; on the contrary, I shall pass on at once to formulate certain empirical propositions which could be supported by experiments, and shall try to show how the analytical propositions are connected with the empirical ones. It will of course be clear that the latter can not be offered as evidence for the former; they are offered as an explanation, or part of an explanation, as to why the definitions (from which the analytic propositions follow) have been accepted as working rules in our language.

2. *The Analytic Propositions.* (1) If a group of objects of a certain kind is counted according to the rules of counting, the result will be the *correct* number of the objects in the group.

(2) A group of objects of a certain kind can be recounted, by the same or by different people, as long as the group remains stable; and, if the counts are *correct*, the result will in every case be the same number; if two counts of the same group do not give the same number, they can not both be *correct*.

(3) If two groups of objects of the same kind are *correctly* counted apart, then the number of objects in the two groups together can be estimated by adding the number of objects in the first group to the number of objects in the second group.

(4) If two or more counts and/or estimates are made of the same group of objects, and there is a discrepancy between the re-

sults, then either the group was not stable, or the counting is *incorrect*, or the calculation is *incorrect*.

3. *Experimental Counting.* Counting is itself a *procedure*, something to be done in accordance with a rule or set of rules. Such a procedure is taught by examples—by showing what it is we do when we count. It can not be said to have been learned until it can be applied to cases not given as examples; it is essentially applicable to new situations.

Let us begin with mere *matching*. On the one hand we have a group of objects—a pile of books, for example; on the other hand we have a "standard set" of labels, fingers, letters, numerals. We are to teach a child to pair off the objects one by one with the numbers of the standard set, also taken one by one. We might, for instance, use as our standard set the letters of the alphabet, taken in any order and allowing any kind of repetition. The child then has to match them with the books in a number of different piles, returning more than once to each pile; he is to write down the letters he assigns to each of the piles, each time he "counts." We might expect that at first his records would show variations from count to count. But if I keep on showing him what it is I want him to do, he would no doubt gradually arrive at the point where he always writes down the same number of letters for the same pile of books. And in fact the great majority of children over a certain age can in this way learn to record those results which an overwhelming majority of instructed adults would record. In short, we should all reach agreement; we should achieve a conclusive result.

A second lesson would be to match the books with the letters taken in their conventional order, *A B C D*, etc. The conclusive result now reached will be that everybody (or nearly everybody) writes down exactly the same letters, beginning with *A*, every time the same pile of books is counted. Consequently it will thereafter be sufficient if only the last letter is written down to stand for the result.

The third lesson is to replace the letters by the numerals, taking them also in their conventional order, 1 2 3 4, etc. Again, the last numeral assigned is to be written down as the result. And those of us who are over the stage of infancy, and are not mentally deficient, will in fact agree, in an overwhelming majority of cases, as to the numeral to be assigned to a given group.

4. *Definitions.* (1) By "following the rules of counting," I shall mean following the procedure last described. And that the great majority of people can learn to follow this precedure, and that they usually reach a conclusive result in doing so, are matters

of common observation. It is, for example, a matter of observation that we reach the same result, even though we do not select the *objects* for counting, in the same order. No matter how the objects are shuffled, we reach the same result by counting them.

(2) Where two independent counts assign the same numeral, I shall say that they "confirm each other," where different numerals are assigned, that there is a "discrepancy." That the same numeral is assigned in two different counts is a matter of observation, that they are regarded as confirming each other is a matter of inference or procedure.

I have indicated that it will be exceptional (but does sometimes happen) that two counts should not confirm each other, when both are made by people who know how to count, and where the group does not seem to have changed. We must not say that any such incident is a case of "incorrect counting." But a discrepancy would lead us to make independent tests to find out whether the people really did know how to count, and really did count in accordance with the rules on the particular occasion. If this revealed nothing, we should naturally try to find out whether the group had in fact remained stable.

(3) But can we ensure that a group remains stable except by having recourse to a recount? Obviously we can take ordinary precautions to make sure that the objects to be counted are not interfered with between the counts. Where, for instance, a specimen plant or culture is to be observed again and again in a laboratory, the precautions taken are independent of counting. It is to be noticed, however, that what is meant by saying that a group "has remained stable," is that none of the objects has ceased to be recognizable as an object of the kind that is to be counted; that, in the case of a pile of books, none of the books has been removed, or has suffered such alteration that it could no longer be recognized as a book; and (of course) that no books have been added from outside, or formed by the division of the books there. That spare leaves or dustjackets should be removed or added will not affect the stability of the group—that is, its character as a group of books. And this implies that a test in counting books is not only a test in the assignment of numerals, but a test also in the application of the term "book."

That a group has, so far as can be ascertained, "remained stable," will be taken to mean that observations, independent of counting the members of the group concerned, show with probability that none of these objects has ceased to be an object of the kind that is to be counted, and that no such object has been added, between the counts.

5. *Experimental Counting and Estimating.* We have now to show that a procedure more complicated than simple counting can also be taught and that its results show a comparable regularity.

A group of objects is divided into two or more sub-groups and children are asked to count through each one of a set of such sub-groups, and also to count through the whole group. The same group is then re-divided into a different set of sub-groups and new counts made. This can be done again and again, and each time the children are told to add up the numbers in the sub-groups and to compare the total (which I shall call an "estimate") with the number reached by a count through the whole group, and with numbers reached by estimates based on different sets of sub-groups.

The result would no doubt be that, after practice in addition and in the new procedure as a whole, the great majority of children over a certain age would soon achieve results confirming each other and confirming their through-counts. Similar experiments could be made to introduce multiplication, division, and subtraction into the making of estimates.

Where, in these procedures, we have a discrepancy, we have a new line of investigation open to us. For a discrepancy may be traceable to the calculation—the addition, multiplication, etc. The check will be to compare the result of *their* carrying out such an operation, with the definitive tables of multiplication, addition, etc., or simply to work the calculation again ourselves. As in the case of simple counting, experience shows that discrepancies are, as a general rule, traceable either to some departure from the rules of arithmetic, or from the rules of counting, or to some instability in the group that is being counted.

We might ask "Why is it that a child who has assigned the numeral 5 to the members of one sub-group, and the numeral 3 to the members of another, should, as a general rule, assign the numeral 8 when he counts through the two sub-groups together?" This, in effect, also raises the question whether the numerals assigned in the manner I have described are really the names of numbers—as a reference in the present paragraph appears to take for granted.

The answer must be given by considering the sequence of cardinal integers. In this sequence, a place is found for the result of adding any member to any other member or members; and any number, n, occupies the nth place in the sequence. That n occupies the nth place is of course to be determined by a count; we must count the numbers as we might count the letters of the alphabet in their conventional order. It may sometimes happen that there is a disagreement as to whether H is the 8th member or the 9th; and

there might also be disagreement as to whether 27 is or is not the 27th member of the sequence beginning with 1. Such a disagreement might be traced to the procedure of counting in the same way as in a disagreement as to whether this is the 27th book on the shelf. But it might be traced to the way in which the numbers were deployed—to the carrying out of the procedure of rehearsing the sequence. It could not, of course, be traced to instability in the objects counted, in the sense I have used above. But, in general, we very easily reach agreement on such questions; in this field also we easily arrive at conclusive results. So that whatever set of subgroups of n objects is selected as the basis of an estimate, it will normally be true that the sum of their numbers is the number that would be reached by matching all the objects successively against the members of the sequence: 1 2 3 4 . . . .n. That is to say, it is the number that would be reached by a through-count that followed the rules. Corresponding remarks hold of multiplication, etc.

6. *Following the Rules and Getting the Correct Result.* In my empirical statements about counting I have had to avoid the notion of "the correct count" or "counting correctly." For if I had introduced it I could hardly have presented my statements as empirical; they would have appeared as analytic—in fact, as the analytic propositions given in my second paragraph. Therefore I have been obliged to distinguish between "following the rules of counting" and "correct counting." The former is a procedure that can be taught and which in fact leads to conclusively agreed results when applied in many fields. And it seems to me that we should not accept a count as *correct* unless it fulfilled both these conditions: (1) It is carried out according to the rules for matching objects and numerals. (2) It can be confirmed by other such counts and/or estimates, so long as the group remains stable.

It might seem that the first condition alone is sufficient; that if you follow the rule of counting you must get the correct result, and hence a result that will stand any amount and kind of testing. But of course this is a tautology; unless you get the correct result, we shall say that you can not have been following the rules of counting. Now the empirical account of the procedure allows us to assure ourselves that a given person knows how to count, and that he does count when told to do so; that the results of his counting have been confirmed in the past by independent tests—even that he is "infallible" in the sense in which a good bank-clerk may be—all this, and yet we are not entitled to say: "If he counts this group then he must get the correct result." In other words, a man may, so far as we can ascertain, follow the rules of counting and yet not

get the correct result. The result he gets may still sensibly be tested to see if it is right or not.

And the test of this count must, of course, be the same as that which led us to say that he had become infallible, and the same as that which led us in the first place to say that he could count. At each stage it is a test of *the result* of his carrying out the operation, or applying the rule. The test may be brought to bear on the last step only—on the total he offers, or it may be applied step by step as he counts. In all cases, what we test is whether the result he gets from applying the rule is the one which "everyone" would get, i.e., the one which commands general assent, which has been, or (as we believe) could be, established conclusively by different observers and by different equivalent procedures.

Therefore "the correct count" can not be defined as that which a given person would reach by following the rules of counting to the best of his ability. It must be one which is conclusively established, i.e., (1) which is reached by all instructed persons, following the rules of counting; and (2) which is reached by all equivalent methods of counting and estimating; and (3) which can be confirmed at any stage by general agreement.

The rôle of agreement or confirmation is seen more clearly if we imagine for a moment that in a given case or class of cases it was not possible to establish one and only one agreed result, but that opinion was equally divided between two results. It would be pointless then to say: "But the rules of counting determine which is correct." For each party would claim that their result was reached by the rules of counting; nor is there anything in these rules to decide (for example) whether there are 66 or 67 sheep on the hill. It is not the rules themselves, but the way in which they are applied, that determines a result. In fact, in counting and in estimating the number of objects of a given kind, conclusive results can be obtained in a wide field, by our following of the rules of counting. But this is a fact of experience and is not guaranteed by any rules.

That the conclusive results allow of complete interchange between through-counts and estimates based on counts of sub-groups, clearly implies that we can reach conclusive results in the relevant arithmetical calculations. In the field of addition, multiplication, division, and subtraction of the numbers of counted sub-groups, "conclusive result" has a tolerably clear meaning. If, for example, I give several accountants a very long column of figures to add, there might be disagreement as to the total on the first attempt. And then which (if any) is correct, is not to be decided by appeal to the rule of addition. It is decided when they all reach agreement on subsequent attempts. Or, to be more exact, when a total

on which they all agree has been further confirmed by any other methods of calculation we can think of, which also give conclusive results.

It is important to notice, however, that the congruence of estimates with through-counts, and of estimates with each other, arises out of the general agreement reached in counting the numbers themselves, in the sequence of cardinal whole-numbers. It rests, in other words, upon an experimental fact about the number-sequence; and so long as we had a method of counting the numbers themselves which ensured general agreement that n is the nth member of the sequence, arithmetical equiyalences would be achieved no matter what method we used in the counting *of objects.*

7. *Discrepancies: Deadlock.* Since conclusive results are in fact obtainable in these fields, any discrepancy between through-counts and/or estimates in a group that appears to have remained stable is a sign that the rules have not been followed, or that the group (in spite of appearances) has not remained stable. But, of course, there may be other signs that one or other or both of these alternatives has occurred. I might find by further trial that one or more of the observers can not count according to the rules, or can not add. I might find evidence that one or more of the objects in the group have been removed, or that others have been added. A discrepancy indicates that something is wrong and suggests lines of enquiry to determine *what.*

It is especially easy to explain a discrepancy by discarding an earlier count. Suppose a very large group is counted many times; on the first few counts one figure is reached, but then, on the subsequent counts, another figure is reached again and again without variation. What could lead us to say: "The first figure was correct at the time it was reached—the group has changed!" The first figure can not be further confirmed now, for the counts now give a different figure. Only new special evidence that the group had not been stable could now suffice to rehabilitate the earlier count. In the absence of such independent evidence, the discrepancy will not lead us to say that the group has changed.

However it is always possible to imagine a case in which there is every appearance of an irresolvable deadlock. Suppose the earlier counts were themselves very carefully checked at the time—even recorded on a talking-picture, so that we can watch the counting while it is being done and long after. Suppose also that the laboratory precautions against any interference with the group have been scrupulously carried out. And suppose that recounts now show a different result—recounts carefully scrutinized and many times repeated.

The definition of "correct counting" requires us to say that both counts can not be correct counts of the same group. Either one or both counts are incorrect or the group has not remained stable. The first task is to find out which has happened; but *ex hypothesi* there is at the moment no *other* evidence to show that either of the alternatives is true. What would be the way out of this dilemma? There must be a tug-o'-war between the two theses, "The counting must be correct because it was so carefully scrutinized" and "The group must have remained stable because it was so carefully guarded." If the evidence for the one thesis can in some way be shown to be stronger than the evidence for the other, then a solution can be reached.

In this tug-o'-war, the earlier counts suffer from the disadvantage that they can not now be directly confirmed. If we discard them, it will be in the hope that some day new evidence might turn up to impugn the reliability of those who carried out these counts. And the thesis of the stability of the group is also under a certain disadvantage, for according to the counts nothing is clearer than that the number of objects has changed; and these counts are not all the work of one observer. If we decide that the number has changed, we may hope to confirm this view some day by finding other evidence that objects of the particular kind counted do "come and go" in a way that eludes the ordinary laboratory control.

8. *The Application of a Rule: The Making of a Rule.* The question that is being asked is "What is the correct way out of the deadlock?" or "What is the correct inference to make from this extraordinary state of affairs?"

It is a question of procedure: "What rule is to be applied here?" It seems that there are two inferences that might be drawn and we want to know which is correct. The answer expected would itself be a rule; nothing but a rule could determine which of the alternatives is correct.

It is possible, of course, that the rule determines that neither is correct, and lays down a quite different sort of action. Perhaps the correct inference to make is simply that one or other of the suggestions already made is correct but that we can not determine which. It would not be surprising if, in this highly unusual situation, we should expect one kind of solution, but finally decide that quite a different sort of solution was correct. But the "Either . . . or . . ." solution has the great disadvantage that it does not provide a "decision" in the sense we expect of empirical propositions.

It is possible that there is no rule at all for such an extraordinary situation. In this case, any solution that was attempted would be generally accepted only if it somehow commended itself

to competent people and was adopted as a rule. In other words, this may be a situation calling for new judge-made law. If we suppose that both the theses (the stability of the group and the reliability of the counting) are well, and strictly equally, established, then I think that there is no *correct* way out of the dilemma. That is to say, I think it would not be incorrect to say that the group must somehow have changed, and not incorrect to say that the counting must somehow be wrong. But two things of importance emerge.

First, that the possibility of a decision rests upon the possibility of saying, in respect of two counts apparently of the same group, that they are *not* of the same group; i.e., upon the possibility of *not* identifying two counts whenever they are discrepant. Upon the possibility of identifying congruent counts and distinguishing between discrepant counts, the whole notion of "correct counting" rests. And this is a matter of procedure. If we never adopted this procedure we should never be able to find out by recounting whether or not a group of objects had remained stable. And while we have discussed methods, independent of counting, by which we can arrange beforehand that a group shall remain stable, a recount is the most useful and obvious means of checking such arrangements and of establishing the fact of stability where no such precautions have been made. So that it seems to me that, in the absence of the procedure of recounting, our notion of a continuing object would be very unlike our present notion. But one could easily imagine objections that might be made against the procedure of identifying the results of two independent counts. Is not every independent count a *process* as well as the carrying out of a procedure? Is it not a series of events having its own unique result, determined by its own unique circumstances? A full discussion of this point would need to consider a great many other identification-procedures of empirical language, including those examined by Kant in the Transcendental Aesthetic.

Second, if we adopt either inference as valid it will conflict with the alternative, but we may still decline to recognize this conflict as a case of inconsistency. If we say that the counts are correct, we determine by our procedure that the counts are *stronger* as evidence of instability than the assurance of our own observations as evidence of stability. If we say that the group has remained stable, on the other hand, we rule that our observations are stronger evidence than our counting. In other words, our solution (if we come to any solution) is achieved by assigning an *ad hoc* order or priority to the various requirements of a "correct count." Such a ruling might nevertheless be accepted as the natural and inevi-

table procedure; for, in the last analysis, it is for the users of the language themselves to determine in concrete cases what they mean by "consistency" and "inconsistency."[1]

KARL BRITTON

UNIVERSITY COLLEGE, SWANSEA

COMMENTS AND CRITICISM

SIGNS, INTERPRETANTS, AND SIGNIFICATA

In the present paper. I propose to consider certain aspects of the conception of the sign-process outlined in Charles Morris's recent book, *Signs, Language, and Behavior,*[1] in the light of questions which an intelligible account of this process must answer. The analysis will focus about two issues: (*a*) Does Morris's theory come to terms with these questions? (*b*) Can the questions be answered without serious modification of the principles of the theory? The formulation of a warranted judgment on this matter requires a rather careful analysis of Morris's views. If this judgment turns out to be critical, it should not be assumed that the writer is unappreciative of what Morris achieves in his book or unaware of the difficulty of the project he undertook.

In *Signs, Language, and Behavior* the basic terminology of "semiotic" is identical with that employed in the *Foundations of the Theory of Signs,* with the exception that Morris substitutes for the terms "designatum" and "designates" the terms "significatum" and "signifies." The assumption concerning the basic semiotic factors has not undergone significant alteration; these are sign, interpretant, and significatum. It is about these factors internal to the sign-process, their nature, their relationship, and function within the process that the questions referred to above are concerned.

In discussing the interpretant Morris introduces the expression "disposition to respond." There are "dispositions to respond" which are not interpretants although there is no sign which is not functionally correlated with such a disposition. But, if a sign has an interpretant, which it must, its interpretant is such a disposition. When correlated in the required manner with a sign such a disposition is an interpretant. Being an interpretant re-

[1] In writing this paper, I have benefitted much from previous discussions with Professor L. Wittgenstein and Mr. Rush Rhees. Neither of them is to be held responsible for what I have written.

[1] New York: Prentice-Hall, Inc., 1946.

quires that such a disposition enter into certain relations with other factors.

Now the question of the character of this relationship is a critical one. But consideration of it in the light of Morris's thinking will be deferred. For the time being attention will be focused on the assumption that the interpretant *is a disposition to respond.* Our initial interest is not that of challenging this assumption; it is rather that of determining, if possible, what it implies. The question of what it implies is particularly critical, since Morris suggests that "interpretant" is synonymous with "idea" (p. 30).

One thing made clear in the discussion is that "disposition to respond" in certain ways is to be distinguished from a stimulus-response-sequence. The "disposition" may manifest itself in a concrete, overt response, but it is by no means necessary that it do so. Hence, whatever a "disposition to respond" may in fact be, it appears that the words, if they have any identifiable referent, must refer to some "state" or "set of circumstances" occurring within the organism. Assuming that this is the case, it becomes pertinent to ask, is a disposition to act a cortical phenomenon? If not, is it a peripheral phenomenon?

These questions need elucidation, if the issue is to be comprehended. The first may be broken down into three. (*a*) Is the response disposition a determinate patterned cortical process subserved by a determinate organization of cortical elements (which may be conceived as the cortical surrogate of antecedent acts of a like character), the occurrence of which does not necessarily innervate any set of motor response mechanisms: i.e., that does not necessarily incite any muscular or glandular reaction? (*b*) Is it a cortical process of such character, that when it occurs, it necessarily leads to the innervation of motor mechanisms? (*c*) Does the term refer on the other hand to some peculiar property of a cortical process or pattern of cortical elements, which may be thought of as the cortical organization of a system of motor reactions, such as "set," "preparedness to act," "determining tendency," etc.

The second question is simple. It asks, is a disposition to respond a motor affair, rather than a central or cortical one, say, an organized recurrent system of "implicit" responses, or even an organization of motor mechanisms, which when activated determines a given response-sequence? An affirmative answer to this question commits one to a motor theory of the interpretant, to a radical behaviorism.

Despite the fact that Morris's use of the term "disposition to respond" to characterize the interpretant seems to be *prima facie*

evidence of his commitment to a motor or peripheral theory, it is possible that he is not rejecting a cortical theory, but insisting rather that the action or behavior phase of the behavior-environment complex is *represented* by some cortical function involved in the sign-process. If this is his intention he is on sound ground. That the action side of the complex is represented in cortical organization and function is a hypothesis the mass of relevant evidence makes difficult to reject.

There are passages to be found in *Signs, Language, and Behavior* which are not incompatible with this interpretation. And then there are statements which some readers may interpret as suggesting that "disposition to respond" refers to a cortical function of the sort mentioned. But the writer finds no positive evidence suggesting a cortical theory of the interpretant, that the action phase of the behavior-environment complex is represented by a cortical function, or that any cortical function is implicated in the representation of the action phase. On the contrary, it is difficult to see how Morris could legitimately reject the characterization of his conception of interpretant as a motor or peripheral conception. Whatever *disposition to respond in certain ways* may refer to, it does not appear to refer to any process or function of a cortical nature.

The writer would not argue that Morris is open to criticism for identifying the interpretant of the sign with a disposition to respond and for not identifying a disposition to respond with a cortical function. He would argue, however, that since there is a large mass of evidence to the effect that the sign-process involves as an essential phase a complex and patterned cortical process presumably "representative" of the action phase of the behavior-environment complex, and that since there is considerable experimental evidence supporting the assumption that the sign-process in its higher manifestation, at least, *is* a cortical phenomenon, a theory that ignores this evidence is inadequate, to say the least.

In the light of the evidence bearing on the issue, a motor theory seems at best to be an over-simplification. Whether such a process be called the "interpretant of" the sign or not, the evidence supports the hypothesis that the sign-process involves as an essential phase of itself a complex pattern of cortical processes of motor derivation. It is in terms of such processes and their correlation with behavior, implicit or overt, that we are to understand the organization manifested in behavior as well as such aspects of the sign-process as "expectancy," "futurity," etc. This pattern of processes I have elsewhere called the "response-process." The evidence supporting the assumption of such a process is available to

anyone who will acquaint himself with the current experimental literature bearing on this subject. If Morris is committed to a motor theory, as he seems to be, his theory must be modified if it is to provide the foundations for a science of sign-action.

Since the concepts "significatum" and "denotatum" obviously have to do with the aspect phase of the sign-process, it might be well to begin the analysis of Morris's treatment of the former and its relation to the interpretant by fixing attention on two issues: (*a*) the difference between denotatum and significatum, and (*b*) the reasons for introducing the latter concept.

Let us first note some formulations supposedly clarifying the concept "denotatum."

> Anything which would permit the completion of the response-sequences to which the interpreter is disposed because of a sign will be called a *denotatum* of the sign. . . . So in the example of the dog, the buzzer is the sign; the dog is the interpreter; the disposition to seek food at a certain place, when caused by the buzzer, is the interpretant; food in the place sought which permits the completion of the response-sequences to which the dog is disposed is a denotatum and is denoted by the buzzer [pp. 17–18]. . . . According to this usage of terms [Morris continues], while a sign must signify, it may or may not denote. The buzzer can signify to the dog food at a given place without there being food at the place in question, and the land-slide signified by the spoken words may not in fact exist. [P. 18.]

As these passages indicate, Morris's treatment of the issue is hardly more instructive and certainly no less elementary than that contained in the ***Foundations***. Certain points, of course, are clear. A denotatum is necessary to the completion of the response-sequences to which the interpreter is disposed. It is thus a necessary condition for the carrying out of overt expressions of the action tendencies which constitute the interpretant of the sign. But a denotatum is obviously not something necessary to the occurrence of a given sign-process. It is independent of the sign-process. It transcends it. A significatum on the contrary appears to be something necessary to the sign-process; i.e., a necessary condition of the sign-process, but not a sufficient condition for the overt expression of the response-sequence instigated by the sign. Since a sign must have a significatum even though it may not have a denotatum and has both when it has the latter, the two can hardly belong under the same category. They must be of a different order of being. The question before us then is this: What sort of entity is a significatum? What does the term refer to? Does it refer to something which, like a "disposition to respond," may or may not be a factor in semiosis, and is such a factor only on the condition of entering into certain relations with other factors? Or does it refer to some entity, process, or what not, which, unlike

the interpretant, requires the sign-process as a condition necessary to its being? If Morris can not tell us what the referent of the term, i.e., what a significatum is, it would appear that there is little point in using the term. And this would suggest very strongly that he has not even faced one of the basic problems of semiosis.

Morris warns against the temptation to conceive of the significatum as a "special kind of thing," a subsistent, Platonic idea, or what not, as if such verbalization were anything more than meaningless jargon. And he apparently thinks that he has avoided such a pitfall by asserting that the sign does not denote its significatum, but rather "signifies" it. But to tell us that a significatum, which after all is quite different from a denotatum although in some manner related to it, is not a "special kind of thing," and that a sign signifies rather than denotes it, is hardly to tell us what "kind of thing" a significatum is, or to cast any light on its relation to an action tendency, i.e., to a "disposition to respond." If a significatum is not such a special sort of thing, and is also not the ordinary sort of thing, such as a chair, table, piece of orange, which would permit the carrying out of an act, such as sitting, eating, etc., precisely what sort of thing is it? Morris offers the following elucidation: "Those conditions which are such that whatever fulfills them is a denotatum will be called a *significatum* of the sign" (*ibid.*, p. 17). For instance, if the denotatum of a sign is *food in a certain place,* "the condition of being an edible object (perhaps of a certain sort) in a given place is the significatum . . . and is what the sign signifies" (*ibid.*, p. 18). In other words, if a denotatum of a sign is an orange in one's hip pocket, the significatum of this sign is the *condition* of being an orange in one's hip pocket, or a set of *conditions* which are fulfilled by the orange. But what do the words *"condition of being* an object,[2] *those conditions which are such that anything that fulfills them is a denotatum"* stand for, refer to, unless to a set of characteristics which may or may not be exemplified by some concrete object? If the words are meaningful they must stand for some set of circumstances different from the concrete objects which fulfill these *conditions.* Moreover, since there are significata without there necessarily being denotata the status of significata can not be identical with that of the denotata; they must have some sort of reality different from that enjoyed by denotata. From the assumptions

[2] In distinguishing between *being a particular object,* a denotatum, and the *condition* of being such an object, a significatum, Morris of course is stressing the obvious, namely, the multiple extension function of the average sign.

of the theory it also follows that whatever the words refer to they must refer to some sort of set of circumstances which enters into determinate relations with concrete events, e.g., signs and interpretants. A significatum must be a relatum.

Aside from denying that a significatum—the *conditions of being* a concrete object as contrasted with being such an object—is a unique thing of any sort, Morris fails unfortunately to elucidate the meaning of his critical terms. His treatment of the issue is as ambiguous as that contained in the *Foundations*. His employment of the expression "condition of being" such and such an object as the verbal equivalent of significatum is no more or less enlightening than was his use of the words "class of objects" as the verbal equivalent of *designatum* in that essay. In fact the theory of the *significatum* in *Signs, Language, and Behavior* has not been advanced beyond the theory of the "designatum" proposed in the former work. And until Morris does present an intelligible discussion of the issue it can hardly be claimed that he has laid the foundations for a science of signs. This treatment of the object-phase of the sign-process violates one of the basic canons of scientific discourse.

The ambiguity manifest in Morris's handling of the issue, his failure at any point really to come to grips with the problem, is understandable, however. It is a function of two sets of circumstances. The first is his effort to avoid any form of the unique entity theory. He sees clearly that such a theory, if not meaningless jargon, is flatly incompatible with a scientifically oriented theory of sign-action and will have none of it. The second is his failure to take full cognizance of two facts: (*a*) That the only alternative to some variant of the unique entity theory is a cortical theory, a theory which conceives the object-phase of the sign-process as a cortical "function." (*b*) That the mass of evidence from varied scientific sources not merely supports some such theory but also makes it compulsive. In view of his commitment to a scientifically oriented approach to the sign-process, and especially in view of his attempt to ground both his conception of sign and interpretant on current behavior theory, it is a little difficult to see why Morris has so consistently refused to adjust his thinking to the evidence in dealing with the object-phase of the sign-process.

If the project Morris has outlined in *Signs, Language, and Behavior*, and contributed substantially to therein, is to be carried out it must be recognized that the only theory of the object-phase of semiosis that is either intelligible or scientifically warranted is a cortical theory. Cortical function provides the key to an understanding of both the "response-phase" and the "object-phase" of

the sign-process. The significatum as such is a function performed by a complex cortical process. The only unique object is a "functional-object."

GEORGE GENTRY

UNIVERSITY OF TEXAS

BOOK REVIEWS

Signs, Language, and Behavior. CHARLES W. MORRIS. New York: Prentice-Hall, Inc. 1946. xii + 365 pp. $5.00.

Perhaps the most valuable, because most convincing, parts of this book by an eminent philosopher of language are the last two chapters (VII and VIII), on the "individual and social import of signs" and "the scope and import of semiotic." Philosophers who think philosophy fundamentally is lyrical soliloquizing on the one hand, or involvement in vital value-metaphysics on the other, and who despise any metaphysics-free analysis of sign-functions as inimical to the human spirit, will be surprised to hear Morris say: "An education which gave due place to semiotic would destroy at its foundations the cleavage and opposition of science and the humanities" (p. 246). On the whole, the book defends the humanities against "reduction" to scientific standing, by giving science itself a distinct and coördinate position (neither superior nor inferior) alongside them. It is thus also a defense of science against what we might call the "elevationist" tendency to volatilize it, in its higher reaches, into poetry and religion—i.e., into the humanities.

The book is a little encyclopedia in the field of the science of signs, and this is its chief merit. In a substantial appendix, the reader is introduced to the history (past and contemporary) of semiotic, which he is helped to pursue further by a large bibliography (thirty-two pages). And at numerous points in the body of the text (supplemented by references) he meets psychologists, sociologists, philosophers, linguists, logicians, *via* their remarks on meaning. Furthermore, the body of the book is a painstaking attempt to construct a copious terminology adequate to the discussion of the delicate issues in the theory of signs; a glossary of these key terms (a hundred and four) is appended.

In the glossary, the terms "meaning" and "mean" do not appear. Morris is proud of the omission and has made a point of it. His point is that these terms are hopelessly ambiguous, so in his semiotical system they are taboo. But a close inspection of the system shows that Morris has gained a very small advantage by the omission, since in place of "to mean" he uses "to signify," with practically

the same generality with which the former term is commonly used. Absolutely *any* sign "signifies" something (and this holds for any prelinguistic, linguistic, postlinguistic sign or symbol), just as any sign is usually supposed to "mean" something—this being the minimum condition (definitional) of an event or object having the status of a sign. To be sure, Morris distinguishes carefully between "modes of signifying" to take the poison out of the ambiguity of "signify," and he argues a difference between "signify" and "denote"—some signs do not denote anything. But why not let all signs "mean," and then distinguish two fundamental "modes of meaning," the significative and the denotative (the latter being a condition of the "truth" of significant ascriptors), and finally (as Morris does) making subdivisions of significative meaning into "modes of signifying"? "Meaning" thus would simply be a more general term than "signifying," covering denotation as well, with the poison also extracted from its ambiguity.

This, however, is a terminological dispute. I want to raise a more serious issue, having to do with Morris's treatment of "modes of signifying," which category is basic in his semiotic. The criticism, put simply, is this: since Morris makes the mistake of supposing that modes of signifying are to be distinguished in terms of kinds of things signified ("significata"), he in effect gives us no analysis of the ways of signify*ing* or mean*ing*. And this latter question is the crucial one in the theory of sign-functioning. In short, Morris unwittingly uses "signifying" with the same vagueness or *un*systematic ambiguity that he objects to in the ordinary use of "meaning." Let me now indicate why I fear this is the case.

Here is Morris's statement:

> There are three major factors discriminable in sign-behavior corresponding to (1) the nature of the environment in which the organism operates, (2) the import or relevance of this environment for the needs of the organism, and (3) the ways in which the organism must act upon the environment in order to satisfy its needs. These three factors, when signified, may be called respectively the *designative,* the *appraisive,* and the *prescriptive* components of signifying. [P. 62.]

Owing to a relative independence of these three factors (environment, need, and response-action, p. 63), three special kinds of signs emerge on the human, linguistic level of signification—(1) designators, which signify certain characteristics or the "nature" of objects; (2) appraisors, signifying the "preferential statue which objects have in behavior" (p. 79); and (3) prescriptors, signifying the appropriate response-action or what to do. Morris writes: "Since these three kinds answer the questions . . . what characteristics? why relevant? how respond? they are respectively

. . . what, why, and how signs" (p. 72). Now, signs that can answer questions about the what, how, and why of behavioral situations certainly convey information. Morris does differentiate them further in a manner to be examined later, but the only difference so far is in *kinds of things signified* ("significata"), all apparently *designated* by their respective signs.

This impression, that it is not the ways of signifying that are being differentiated but rather the kinds of things signified, is deepened by Morris's theory of "ascriptors." An ascriptor is an expression containing an "identifior," signifying a position ("locatum") and either a designator, appraisor, or prescriptor as a kind of predicate. All ascriptors ascribe or predicate something, since, according to Morris, appraisive or prescriptive ascriptors are as capable of truth or falsity as the designative sort. He writes: "In all of these cases [of the three different kinds of predications] ascriptors . . . signify that objects have certain properties" (p. 113). So they may be tested for their truth:

> If the ascriptor is designative, the evidence is that objects have the characteristics which support the response prepared; if the ascriptor is appraisive, the evidence is that the interpreter of the sign accords to the object or objects the preferential behavior to which he was disposed by the sign; if the ascriptor is prescriptive, the evidence is that the interpreter of the sign does act to the object itself in the specific manner he was disposed to act because of the sign. [P. 113.]

I am not at all clear as to what Morris means by the above analysis of three kinds of confirmation, not even after carefully examining his illustration of dog-and-buzzer. He says that, to a dog appropriately conditioned, the buzzer simultaneously *designates* certain properties of an object, *appraises* it, and *prescribes* a certain response-action. That is, the buzzer disposes the dog to expect certain visual, olfactory, and gustatory "sensations," etc. (qualities of the dinner at a certain place), discharges a disposition to prefer or like it, and touches off a motor-disposition to behave in a certain way (going to the place of eating). Now it is fairly clear how, in the designative dimension of the buzzer's significance, experiencing the expected qualities of the dinner would "support" the prepared response, by a kind of "closure"; this indeed involves evidence and confirmation. But we get into difficulty in the other two dimensions of the buzzer's significance, if we import the idea of proof and evidence into them. Just what is proved, and how, in the response to the buzzer as an "appraisor" or "prescriptor"? What Morris says above about this seems to convert (if not "reduce") the appraisive significance into a *designative* ascriptor, of the form "You will like *x*," since it is only

such that gets confirmed or falsified by an actual liking or not liking. Similar remarks hold for Morris's prescriptors. It is some propositional or factual meaning-content of a command or imperative that gets confirmation through action, if proof and truth are involved in it at all. Thus they too are reduced to designative significance. (Morris's above pronouncement on the alleged special kind of "prescriptive truth" is unintelligible to me: " . . . the evidence for the prescriptive ascriptor is that the interpreter of the sign does act to the object itself in the specific manner he was disposed to act because of the sign.")

That Morris is himself aware of this reductive tendency, latent in his truth-theory of appraisors and prescriptors, is indicated by the conclusion he reaches in the section on "inter-relation of modes of signifying," namely, that " . . . prescriptors rest on appraisals and appraisals on statements [designations] in a way which statements do not need to be followed by appraisals and appraisals by prescriptions" (pp. 89–90). This, by making all significance convertible to or (if not this) dependent on factual statement, seems to be leading Morris into the sort of "scientism" that he elsewhere wisely disclaims. The reason behind this trend is excellent: Morris wants all behavior to be intelligent, and this requires that it be kept under the control of *knowledge,* with its designative and confirmable sense. But such a result can be secured without blurring the distinctions between unique *modes of signifying.* This confusion was a pitfall Morris could have avoided had he not implicitly identified the latter with *kinds of things signified.* For him, all these different kinds of things are, in the last analysis, *designated.* We are left with only one mode of *signifying.*

We have perhaps strained and obscured Morris's point by staying too long with the buzzer illustration. In our language, the special functions (designative, appraisive, and prescriptive) are comparatively more separate and independent (p. 89). We have special signs and linguistic forms to accommodate each. "That is rain," "That is good," "You must do it." But, as we have seen, even out of the last two Morris teases true or false predictions *designating* some preferential status or some action (you will like it, you will have to do it thus-and-so, etc.), in accordance with his all-embracing behavioristic philosophy and psychology of meaning and truth. So here again is the conversion of all modes of signifying to a basic one, the designative; or, if not this, then at best we are left with various modes not clearly distinguished or named; and these different ways in which signs signify still call for analysis.

That Morris in these pages partly intends and tries to formulate this crux of any adequate theory of meaning (the different *ways* in which signs signify, *not* the different kinds of things signified) is evident from other remarks of his. "The appraisor . . . *as such* neither identifies an object . . . nor characterizes an object" (p. 81, my italics). "'Good' is an appraisor in so far as it disposes its interpreters to preferential behavior, and is a designator in so far as it signifies merely that some organism has in fact certain preferences" (*loc. cit.*). He speaks of prescriptors as "evoking" responses (p. 107). Now, in this vein, there is a hint of a fundamental mode of signifying (with its own sub-divisions) which might be called meaning-by-contagion or the evocative sense of signs—a mode of signifying to be distinguished from the designative and denotative, or from the "cognitive" function with its truth-value. The special psychology and logic of this highly significant though non-cognitive functioning of signs is mentioned only by innuendo in this book. Such as is formulated is marred by fusion with the theory of the cognitive (designative-denotative) function. That any sign (or any except certain technical signs such as in physics, etc.) functions simultaneously in several dimensions of "sense" (cf. the French "sens" = direction) is a fact that Morris recognizes, but he has failed to carve the subject-matter at its joints. Signs can signify by a kind of discharge-action, touching off or kindling a bit of behavior, a mood, a spectacle in the field of the imagination, all through the evocative power of sign-stimuli. Though such functioning is ordinarily accompanied by (sometimes even dependent on) the cognitive function, it is not *as such* (to use Morris's own expression above) to be assigned a designative-denotative dimension of significance. Through it, signs have "local color." Its virtue is the flavor of private background, province, and special tradition or culture. To translate such meaning into another language is practically impossible, while the cognitive component of the meaning of signs is precisely that which is preserved in translation. (Of course, one who is at home in the two languages can arrange a translation carrying some of even the evocative impact of the original, provided he is poetically gifted.)

The reason for Morris's not developing this real distinction between modes of signifying is not far to seek. His general pragmatic-behavioristic philosophy of meaning resists it. This position makes *all* significance an affair of dispositions-to-respond, discharged by the occurrence of signs as stimuli. Even the *cognitive* component of meaning of, say, "food" or "rain" is defined or determined, according to Morris, by the kind of conditioning

and local discharge-action that is in fact involved only in the *non-cognitive* functioning of signs. The power of language, in its cognitive use, verifiably to formulate general facts and to serve as a critique of "dispositions" of *any* sort, is not accounted for by Morris's theory. Dewey's theory of meaning fails, and vacillates, in the same way, and it is largely under the spell of this instrumentalist position that Morris hesitates to make vigorously the required distinctions. The book as a whole, however, rewards the careful reader, being nothing worse than a near miss. Morris's position is encyclopedic and comprehensive. Though with this goes the tendency to make inadequate distinctions, it nevertheless encourages and helps one to make progress in his own thinking about the subject. The issues raised here are crucial—so fundamental are they that Morris realizes that their adequate treatment requires coöperation among philosophers. There is unfinished business at hand, and we may be grateful to this book for suggesting directions in which solutions lie.

VIRGIL C. ALDRICH

KENYON COLLEGE

La Connaissance dialectique et l'Expérience existentielle. GERMAINE VAN MOLLE. (Bibliothèque Scientifique Belge, No. 40) Liège: Georges Thone. 174 pp.

Although the author calls her approach to philosophy "existentialist," her way of thinking bears little resemblance to that modern strain of Kierkegaardian thought which generally claims this title for itself. "Classical rationalism," which is another word for Cartesian idealism, furnishes not only the foil against which the argument is developed. It also provides the conceptual tools and prescribes the mode of procedure. The exposition is rigorously deductive in form. It never stoops to historical reference, controversial asides, or empirical illustrations but moves at a steady pace in the rarified atmosphere of metaphysical abstraction.

Classical rationalism is a philosophy which takes its departure from the mind considered as an autonomous entity—the Cartesian *ego cogitans*. Building on this foundation it erects a coherent conceptual structure, conceived as the creation of the rational self (pp. 33, 75). This philosophy, the author holds, is not simply wrong. It is in error only if it is mistaken for an unshakable and ultimate position. Divested of this pretension to ultimacy, it becomes a legitimate part of a more comprehensive philosophical enterprise. Under the title of "regressive dialectic" it is given a place as one of the units within the triad of dialectics which according to Mademoiselle Van Molle constitutes philosophy.

Against the idea of the mind as the independent and creative source of rationality the writer sets her existentialist thesis described as "the ontological affirmation" (pp. 15, 21, 25 sq., 68, 111). The axis, so to speak, of the intellectual cosmos is transferred from man to God, and the anthropocentric system is replaced with a theocentric one. God, the ontological affirmation reads, is indeterminate existence of superabundant virtue.

This basic affirmation is not arrived at through discursive reasoning but it expresses an immediate experience. At the margin of its rational deployment, the mind has a confused awareness of an existence of absolute validity which as such is its own, the mind's, foundation (p. 14). For the Cartesian *cogito, ergo sum* the author substitutes the following absolute postulate of discursive thinking: "I am, in being concerned ("*en me sonciant* . . .) about my indetermined and constraining origin, or about absolute and creative existence" (p. 20).

In making the ontological affirmation, i. e., in positing God, one abandons the point of view of regressive dialectic for which the object is created by the mind engaged in rational self-deployment. Instead the mind is viewed as spontaneously expressing what it receives from a source other than itself. Developing the ontological affirmation the mind understands its own existence as originating in an act of Divine self-giving. God, indetermined and, at the same time, superabundant existence unfolds Himself into the individual (human) consciousness. And this consciousness is free to conceive of itself (erroneously) as the autonomous and creative source of rationality. In order to deliver itself from this error, the mind must dislodge "sovereign reason," replacing it with "disturbed reason" (*"raison ébranlée"*). This it does under the constraint of an existential experience, described as "metaphysical unrest" (p. 122). It is this forceful and mysterious "presentiment" which impels the mind to make the ontological affirmation. Developing this existential *a priori*, we come to revise and counter-balance the "regressive dialectic" by a "progressive dialectic." Submitting to God as the indetermined source of all determinateness, we become aware of the derivative and dependent character of human reason.

While progressive dialectic takes its inception from the affirmation of God, it does not have God for its object. The direct inquiry into the nature of God is a task assigned to a third type of argument, called "transcendental dialectical." This third and final dialectic seems to undertake the impossible. How can God's transcendent reality, defined as "indetermined existence," be expressed in the language of categorial determinations? In fact,

the author concludes, all that reason can hope to attain by this its final effort is in part a negation, in part an approximation, twice removed from its object. The negation is given an affirmative expression in the thesis of the "transcendental indiscernibility" of transcendant reality (p. 151). But the insuperable inadequacy of all attempts at a definition of God does not cast discredit upon the authentic, spontaneous, and ever renewed endeavor to clothe in formulae the informulable presentiment of God. By the nature of the case, these formulae can be no more than analogical expressions of the presentiment itself. Since this presentiment in turn is analogically related to its cause, we may affirm that rational theology, or, in the author's language, "transcendental dialectic," must be content with achieving "the analogue of the analogue of God" (p. 148).

While admiring the brilliant qualities of this strenuous little book, the present reviewer finds it hard to come to grips with the writer's ideas. The blame for that difficulty may partly lie with the uncompromisingly abstract form of exposition. The real barrier, however, is in the author's thought rather than in her mode of expression. She takes for granted things which need not be a matter of course for the reader. The chief assumption which we hesitate to accept is that of the limited validity of "classic," or as we should rather call it, "idealistic" rationalism. Yet for the author's argument this assumption is of cardinal importance. Mademoiselle Van Molle's field of vision is somewhat restricted by the very tradition which she undertakes to modify in the light of an existentialist experience. True heir to the Cartesian tradition that she is, she finds no occasion, in her discussion of metaphysical principles, to take account of the psycho-physical nature of man or the relevance to her problem of the I-thou relationship.

HELMUT KUHN

UNIVERSITY OF NORTH CAROLINA

BOOK NOTE

Newton Demands the Muse. Newton's "Opticks" and the Eighteenth Century Poets. MARJORIE HOPE NICOLSON. (History of Ideas Series, No. 2.) Princeton: Princeton University Press. 1946. xix + 174 pp. $2.00.

That Miss Nicolson has made a scholarly and substantial contribution to a little-known segment in the history of ideas will hardly be questioned by anyone. But whether one finds her book an exciting contribution to our understanding of a great period,

or merely a rather curious inquiry into the vagaries of some well-forgotten eighteenth-century poets, will depend partly upon one's attitude toward the Age of Reason and its typical exponents, and partly upon one's sentiments concerning the proper relations between art and science in any age. I, for one, share Miss Nicolson's not uncritical interest in this epoch, "more curious than devout." And I found it very refreshing indeed to contemplate a generation of poets who did not withdraw from the greatest challenge of their epoch, but on the contrary welcomed it as "a dear and genuine intimate of the household of man."[1] Far from being regarded as the enemy of the muse, science provided the eighteenth-century poets with a whole new subject-matter, with new and arresting metaphors, and with an enriched and more meaningful symbolism of the visual sense. Old figures of speech, inherited from more ancient traditions, were invested with a significance and vitality they had lost through overuse. But more important than this, the whole conception of an overarching science of nature, which could explain the whole panoply of experience in terms of a few "laws divinely simple," was eagerly welcomed as a spectacle of incomparable sublimity and imaginative appeal.

The influence of Newton's *Opticks* upon the literature of the period following was extremely varied. Miss Nicolson traces with great skill how this influence made its effect, not only upon the imagery and symbolism of poetry, but also upon the far flung esthetic and metaphysical implications which were associated, rightly or wrongly, with the *Opticks*. Color and light, in fact, dominated this poetry and its esthetic, just as sentiment and emotion were to dominate that of the ages which followed. The poet Akenside, in his *The Pleasures of the Imagination*, even went so far as to identify color with beauty.[2] But perhaps it was Thompson, in

[1] Quoted by Miss Nicolson, p. 56.

[2] We may see here, incidentally, how wide of the truth is Professor Northrop's singularly naïve contention that not until Prall and Georgia O'Keefe were the theory and practice of the "esthetic continuum" properly exploited in Western Civilization. Indeed, the remarkable thing about the eighteenth-century poets was their avid appetite for the esthetic and spiritual values of the sensuous surface of things, as well as their feeling for the close correlation between reason and sense. It did not seem to them at all incongruous that Newton should give color back to poetry "from which it had almost fled during the period of Cartesianism." As Miss Nicolson points out, "there is no question that the first effect of Newton's resolution of the colors and his careful analyses of their properties was to produce a new scientific grasp of a richer world of objective phenomena peculiarly sympathetic to poets" (pp. 21–23). Whole poems were devoted to descriptions of the "feel" and "texture" of the esthetic orders of hue and light. Thompson and Savage, for example, gave particularly lush descriptions of the "gorgeous

the *Seasons*, who best exemplifies the impact of science and its subject-matter upon the poets of the period:

> O unprofuse magnificence divine!
> O wisdom truly perfect! Thus to call
> From a few causes such a scheme of things
> Effects so various, beautiful and great.

Miss Nicolson returns to Thompson again and again for illustration of the rich and diverse effects which Newton's *Opticks* had upon the literature of the eighteenth century. He was a "Newtonian" descriptive poet; but he was a "scientific" and "philosophic" one as well. For him Newton's *Opticks* was a veritable source-book of esthetic and metaphysical associations.

Not all of the scientific poetry of this period, of course, is good. Miss Nicolson makes it quite clear that a great deal of it is merely tedious versifying imbued with no genuine poetic impulse whatever. Nor can it be said that all of the versifiers understood the writings of "the incomparable Mr. Newton." Much was read into him that he would have disdained as the veriest speculation and "hypothesis." But the fact remains that the eighteenth-century poets welcomed Newton's demands upon the muse, and admired his work with something approaching superstitious awe. As Miss Nicolson says, "so far as the eighteenth century poets were concerned Pope did not exaggerate" when he remarked ironically,

> Nature and Nature's laws lay hid in night;
> God said, "Let Newton be!" and all was light.

As this passage indicates, however, the admiration of Newton was not always unqualified. Nor were the effects of this admiration upon those who shared it uniformly beneficial. If "poets, philosophers, scientists may visit each other's countries of the mind with pleasure and profit, sometimes bringing home bags of gold," still "many of these poets overstepped the boundaries too often for their own good." Pope, for all his scornful conservatism, was perhaps a finer poet than any of his more scientifically-minded contemporaries. The point is that while poetry may use science, philosophy, and morality as its materials, its excellence is not necessarily determined by the correctness or even by the kind of views propounded or expressed by the poet. The use of Newton's *Optics* by Thompson and Akenside did not make them worse poets than Pope; nor did it make them any better.

Miss Nicolson concludes her work with an epilogue devoted to

train of parent colors": "the flaming red," "the tawny orange," "delicious yellow," and "all-refreshing green," and, finally, "the last gleamings of refracted light [which] died in the fainting violet away."

the "poetic damnation of Newton" by William Blake. In him "the wheel of fortune will come to full circle." Yet in spite of himself, Blake was also a poet of color and light, and even of Newton's "particles." Again and again he uses them as material and metaphor, even when it is merely to protest against what he thought were their implications. Indeed, Blake, for all his much publicized romanticism, was still a fascinated child of the Age of Reason—which was also the age of "impressions and ideas." And "the atoms of Democritus" also supplied kindling to fire his rare, if imperfect, poetic imagination.

The lesson for esthetics and the philosophy of art, perhaps, is two-fold: (*a*) that the entire world is the material of art, and (*b*) that this world can not become poetry until it has been taken up by the creative imagination and infused with the delight which we call beauty. Poetry is not explanation, any more than it is mere "sensuous surface," though both are proper stimuli to art. But it is surely one of the significant failures of our age that many of its finest poets have felt impelled to turn away both from reason and from sense. Here, as elsewhere, the eighteenth century has much to teach us.

H. D. A.

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NOTES AND NEWS

The newly elected officers of the Western Division of the American Philosophical Association are as follows: President, A. C. Benjamin; Vice-president, D. W. Gotshalk; Secretary-Treasurer, W. A. R. Leys (Roosevelt College, Chicago); members of the executive committee, A. C. Garnett, Willis Moore, Howard D. Roelofs.

The Philosophy of Education Society elected the following officers for the coming year: President, Theodore Brameld; Secretary-Treasurer, William Stanley (University of Illinois); members of the executive committee, Obed Williamson, H. Gordon Hullfish, John S. Brubacher.

John Simon Guggenheim Memorial Fellowships for 1947 were awarded to the following persons in the field of philosophy: Herbert Feigl, Professor of Philosophy, University of Minnesota, "Studies in the field of the philosophical and methodological problems of psychology;" Carl Gustav Hempel, Assistant Professor of Philosophy, Queen's College, New York City, "A study of the logic and methodology of concept formation in empirical science;" Paul Henle, Associate Professor of Philosophy, Northwestern University, "A study of the relations of the characteristics of symbolism to what is expressible in it, with reference to the general theory of meaning."

The Annual Meeting of the Mind Association and Joint Session with the Aristotelian Society will be held at Trinity College, Cambridge University, July 4–6, 1947. The program is as follows: Symposium, "Historical Explanation": *A. M. Maciver, W. M. Walsh, M. Ginsberg.* Symposium, "Rules of Propositions": *Karl Britton, J. O. Urmson, H. H. Price.* Address, "Judging and Choosing": *C. D. Broad.* Symposium, "Does Psychology Study

Mental Acts or Mental Dispositions?": *W. B. Gallie, W. J. H. Sprott, C. A. Mace.* Symposium, "Guilt": *H. D. Lewis, G. Paul, J. W. Harvey.*

The Twelfth International Congress of Psychology (postponed from 1940) will be held in Edinburgh, July 23–29, 1948. For information, address Godfrey Thomson, General Secretary, Moray House, Edinburgh 8, Scotland.

The greater part of the university and other libraries in Poland were destroyed during the German occupation. No books from other countries entered Poland during the war, and the Polish universities can not presently afford the purchase of foreign books. In one philosophical seminar there are now only three books in English; in another, there are none which have been published since 1939.

The philosophical seminars in Poland would appreciate journals and books in English, especially those published in recent years. Books may be sent to the Polish Embassy, Washington, D. C., marked "Books for Polish philosophical seminars."

Helmut Kuhn, of the University of North Carolina, has accepted appointment as professor of philosophy at Emory University.

W. D. Oliver, instructor in philosophy at the University of Minnesota, has been appointed assistant professor of philosophy at the University of Missouri.

Arthur Szathmary, instructor in philosophy at Columbia University, has been appointed assistant professor of philosophy at Princeton University.

Edgar F. Wells has accepted an appointment as assistant professor of philosophy at the University of Pittsburgh.

Hiram J. McLendon has been appointed instructor in philosophy at the University of California, Berkeley.